

Review of “Condensation and immersion freezing Ice Nucleating Particle measurements at Ny-Ålesund (Svalbard) during 2018: evidence of multiple source contribution” by Rinaldi et al.

General Comment:

In this manuscript the INP concentration in Ny-Ålesund (Svalbard) was evaluated in two different seasons and in two different ice nucleation modes, using two different offline techniques. Given the high importance of the Arctic and the low number of studies focusing on INPs, the present study is useful for the ice nucleation, aerosol, and cloud microphysics communities. Although the present results are very valuable, it is not completely clear how this study differs from previous studies conducted at the same Arctic location. The manuscript is poorly written as the authors did not pay attention to several important details as listed below. I encourage the authors to improve the quality of the manuscript taking into account the Major, Minor, and Technical comments.

Major Comments:

1. I do not see a clear difference between the content of the Abstract and the Conclusions. These two sections need to be different with the Abstract including more concise and quantitative information.
2. The English needs to be significantly improved. The way the document is written makes it very difficult to read in some parts. Also, the document seems disorganized, and therefore, I encourage the authors to improve this part. The authors need to be more precise, more quantitative, and improve the statistical analysis.
3. There is key information missing in the text (e.g., technical details, references, units, correlation coefficients, etc.). See below.
4. Lines 94-96: What is different or what is novel in the present study compared to Wex et al. (2019) and Hartmann et al. (2019)?

Minor Comments:

Line 43: Add a reference after “amplification”.

Line 46: Add a reference after “detail”. How about Abbatt et al. (2019)?

Line 53: “sufficient numbers”. Please be clear.

Line 56: “transport dynamics”. Please be clear.

Line 57: Add a reference after “budgets”.

Line 69: Define “T”.

Line 77: “tripling INP”. Please be clear.

Line 88: “evidencing order of magnitude wise increase”. Please be clear.

Line 89: Add a reference after “ice”.

Line 99: “The aerosol sampling was performed at the Gruvebadet observatory”.
Add a map showing it.

Lines 110-111: “The sampling generally started in the morning, during the spring campaign, while it started typically in the afternoon during the summer campaign”.
What is the reason for this?

Lines 110-111: Add a Table with the details of each sample from both techniques (e.g., initial time, final time, date, etc.).

Lines 113-119: When where the samples collected with the WT-CRAFT?

Line 114: “0.2 μm pore size”. Brand? Model?

Line 115: Define “TSP”.

Lines 118-119: If the flow rate of the WT-CRAFT is 150 lpm and the flow rate for the DFPC is 38.3 lpm, why the samples from the former one was 4 days and for the later one just 3-4 hours?

Line 121: The authors need to provide a brief description of the method.

Lines 128-129: I would rather add a small paragraph indicating how good is the agreement of the DFPC data compared to other techniques.

Lines 141-142: “in a known volume”. Specify the volume.

Line 141-142. This is not very clear.

Line 151: No parametrizations were derived in this study.

Line 152: “concentration of ice nucleating particles ($n\text{INP}$)”. This was defined in Line 69.

Line 164: “1.95 g cm^{-3} ”. Add a reference.

Line 167: “air temperature, T”. This was first used in Line 69.

Line 173: “GVB”. Define it.

Line 173-174: “on filters collected”. The authors need to be clear on what filters and how the particle were collected.

Line 177: “ $\text{C}_2\text{O}_4^{2-}$ ”. Fix it.

Line 179: "Mg²⁺, Ca²⁺". Fix it.

Line 211 and along the text: The authors used "at Ny-Ålesund", "GVB", and "Gruvebadet". Please be consistent.

Line 217-219: "t", "L", "C_t" and "D_{ijt}" should be in italics.

Lines 241-242: "The observed offset may derive from the different time resolutions of the sampling for INP analyses, as well as from uncertainties in sampling activities and/or measurement uncertainties". How about the particle size analyzed in both techniques? The pore size of the filters used is different in each technique.

Line 242: In the Hiranuma et al. (2015) paper the 2 techniques were not used.

Line 244: Line 43: Add a reference after "questionable".

Lines 250: Are you sure the authors clearly distinguished between the 2 modes in Wex et al. (2014)?

Line 252: "different aerosol types yielded different results". Again, is it not the size measured by both techniques? Depending on the aerosol type, their size distribution changes, and therefore, the particle collection efficiency of each technique.

Lines 257-259: "median 115". From Figure 1, the value seems to be close to 90 m⁻³ instead of 115 m⁻³.

Line 259: "33-135 (median 77), 18-107 (45) and 6-66 (20) m⁻³". This is not shown in Figure 1. Please add them to the Figure.

Line 264: "24-9082". What is the reason of such large variability?

Line 274: "range 5-10, 10-30 and 30-70". Units are missing.

Lines 281-182: "we can conclude that the results of the present study are generally consistent with literature". Add literature data to Figure 1.

Line 282: Add Wex et al. (2019) data to Figure 1.

Line 294: "similar". By how much?

Lines 300-302: I don't get what is unique here.

Line 304: "shows the bimodal activation with a hump feature at T above -15 °C.". Really?

Lines 304-305: "may be due to marine biogenic aerosols". Are not you sampling in a marine environment? Why is this surprising?

Line 311: "suggesting that the dominant INP sources may be located at long distances". This is rather speculative. Make a link with the evidence you show later.

Lines 313-314: “likely resulting from the activation of local sources after snow and ice melting”. Why coarse particles can be transported long distances in spring and not in summer.

Line 317: Mason et al. (2016) found a large contribution from the coarse particles with most of the samples collected in Alert during the spring.

Line 319: The Creamean et al. (2018) samples were collected in spring.

Lines 321-322: Should “ cm^{-3} ” be “ m^{-3} ”?

Line 325: “at lower temperatures”. Should it be higher?

Line 332: Add a reference after “melting”.

Line 354: “at GVB (2012)”. Add the corresponding paper.

Line 360: Why wind direction was not included?

Line 361: “were often associated to a reduction”. It is not very obvious from the Figure. This is a qualitative conclusion.

Line 361: “the exception of precipitation events”. Add the r^2 .

Lines 362-371: I don’t think this is really necessary as it adds too little to the discussion and does not help at all to support the data.

Line 372: “covariate”. Add the r^2 .

Line 374: “more accentuated” and “significant correlations”. Add the r^2 .

Line 385: “showed a significant”. Add the r^2 .

Lines 388-389: “mainly related to long-range transport of anthropogenic aerosol particles from lower latitudes (Arctic haze)”. No evidence provided.

Line 396: “for all the activation temperatures”. Just 2 temperatures are shown in the Figure.

Lines 396-397: “with the exception of the coldest one ($T = -25\text{ °C}$)”. This information is not provided.

Lines 398-399: “only a minority of samples (<50%)”. 30-40% is a minority?

Lines 399-401: I don’t get it.

Lines 409-410: “suggesting that the INP population over the Arctic in summer originates from a combination of mineral dust and marine aerosol particles”. If I understood correctly, this figure contains the data from both summer and spring, therefore, such conclusion cannot be drawn from the data reported in the figure. Please separate the data sets into summer and spring.

Line 417: “nINPDFPC correlated”. Add r^2 and p .

Line 422 and 424: “anticorrelation” and “significant”. Add r^2 .

Figure 2. It seems you are comparing apples with oranges. Why are the y axis scale different? This figure and its discussion seems to be useless.

Figure 4. The authors need to separate them between summer and spring.

Technical comments:

1. Line 62: Delete the “dot” after “temperatures”.
2. Line 100, Line 168: “Km” should be “km”.
3. Lines 116-117: “diaphragm pump”. Please add the details about the pump.
4. Line 272: “immersion mode freezing”. Be clear.
5. Line 273: “).” Fix it.
6. Line 280: “etc...”. It should be one dot.
7. Line 282: “Both the datasets discussed”. Be clear.
8. Line 287: “while both our datasets”. Be clear.
9. Line 293: “ranged 0.4-15 and 2-40 m-3”. At what temperatures?
10. Line 361: Figure S2 is called before Figure S1.
11. Line 436: Figure S3 is called before Figure S1.
12. The citing format is wrong and needs to be fixed (e.g. missing spaces between references and when multiple references are cite, they are not organized chronologically).
13. The format of the units if not uniform. For example in some cases the authors used “X ° C” but in other cases “X°C” is used. Please be consistent.
14. Figure S1. Change the color of the “snow lines” as it is not clearly distinguishable from the white background.
15. How much time passed from sampling until the actual INP analyses? Add this information to the text.
16. Figure S2. The nINP in the middle left panel should be in blue. The nINP in the bottom left panel should be in black. Add more details to the Figure caption.
17. Figures S2. I am not sure if it makes sense to correlate precipitation and INP concentration when using the WT-CRAFT based on the low time resolution i.e., 4 days.